at the last observation had so closely approached the edge that it was scarcely, if at all, visible, and the entire disc might then be said to appear free from spots of any kind. On March 23 the sun remained altogether clouded. At night, however, the sky cleared, and I had a good view of the occultation of Kappa Geminorum. The disappearance was, as usual, instantaneous, but immediately after it a delicate ray seemed to shoot out from the place of the star in a direction perpendicular to the edge of the moon, and the appearance lasted about eight seconds.

Milbrook, Tuam, March 24 J. BIRMINGHAM

α Centauri

AT the meeting of the Royal Astronomical Society December 8, 1876, Mr. Marth asked for measures of this binary.

As the star never reaches more than 11° above my horizon, the definition must always be imperfect; but the following measures taken on the morning of February 22 appear consistent, and may be useful until better results are obtained from the southern hemisphere:—

Epoch 1877·14.	Distance. ″	Position. °
1	3·4	64·0
2	3·1	64·5
3	3·1	64·3
4	3·7	64·7
Mean	3·3	64·4

The component stars are of about the first and second magnitudes, and their colour is yellow. Power employed, 240; definition very bad. MAXWELL HALL

Jamaica

The Boomerang

WILL you allow me to add my experience of the use of this weapon to that furnished by your other correspondents? My experience is mainly confined to the natives about the Condamine and its affluents, where I was frequently in company with natives for about a year. They had two weapons—one large, for war, the other small, for game. I should think the weapon is seldom thrown in war, since most of their contests (such as they are) take place in scrubb or forest, where it could not be used to advantage; but I have seen a native frightfully cut in the abdomen, and was told by a native that he had been struck by a boomerang thrown by the hostile party. I have seen a few of these contests, but never saw the boomerang used in any way. The "waddys" were thrown freely, the spear seldom. The game boomerang is thrown among flights of ducks, and also parrots when congregated on the trees and gathering nectar from their flowers, and with marked effect. This I have seen several times. There are two ways of throwing the weapon, which, as I could throw it well at one time, I will endeavour to describe. It is grasped quite at the end by the right hand and raised above the head, the elbow being bent, the weapon assuming a position with its convex edge downwards on a nearly horizontal line at right angles to the intended line of flight. The arm is brought swiftly round from left to right, becoming gradually extended until it reaches a line directly in front of the face, when the weapon is delivered from the now straight arm, with the concave edge towards the line of flight. This is the method of throwing into the air. No dependence can be placed on the return of the weapon within a circle of twenty yards, though it sometimes returns dangerously near the thrower. If it meets with an obstacle it is either stopped and falls dead to the earth, or its course is changed. In either case its peculiar motion is destroyed, as must be obvious. In the other method of throwing the weapon is held in the same way, but delivered nearly on line with the hip, and made to strike the earth about ten yards in front of the thrower, pitching, I believe (though it is not easy to observe), on one of its horns. Thence it ricochets and flies straight away for perhaps seventy or eighty yards, keeping a position of about four feet from the earth, and gradually rising until it is spent. It returns very little if at all. In this way only can it be used for war, since in the other it begins to mount at once, and would soon be above the enemy's head. The weapon is made of various woods, a piece with a slight elbow being selected. It is hardened by baking. The right form is arrived at by trial, as I have seen during the process of manufacture. Those sold to Europeans are the failures. I had to pay a good price for the two I brought home, but they were excellent specimens.

The natives drive ducks. A flight is marked down on a small creek; men are then posted along the bank, others drive the birds towards them, and the boomerangs are thrown as they pass. I do not recollect having seen the weapon used for ground game. These are surrounded and killed with spears and sticks. ARTHUR NICOLS

Is Meteorology a Science?

THE recent article in NATURE on the Treasury Blue Book relating to meteorology brings into unpleasant prominence the opinion of the eminent astronomer, Sir G. Airy, that meteorology is not a science; and the evidence of the eminent physicist, Sir William Thomson, to the same effect; while a celebrated philosopher in the columns of the *Fortnightly Review* has not long since described meteorology as "a formless registry of facts."

But surely these eminent authorities have hardly realised the great change which has come over the whole aspect of meteorology since the introduction of synoptic charts?

Synoptic meteorology shows that the world is, broadly speaking, covered with shifting cyclones and anti-cyclones, which have each, subject to local, diurnal, and other variations, a characteristic weather, and physical appearance, and one great problem of meteorology is to explain the observed weather over any area, at any instant, by defining the position of these cyclones and anti-cyclones. It is in fact analogous to that branch of geology which explains the scenery and contours of any country by the position of areas of upheaval, crumbling, and subsequent denudation.

But there is another problem for the meteorologist to solve, viz., Given the position of the cyclones and anti-cyclones at any instant, to determine their future course and changes; and this can even now be done in certain cases. As if the geologist were asked what the future course of the present state of the earth's surface will be, where fresh upheavals or crumblings will occur, and what the corresponding changes in scenery will be? In this case the position of the meteorologist is far in advance of the geologist.

But still another reproach is cast upon meteorology—that the knowledge requisite to issue forecasts cannot be expressed in mathematical formulæ or in simple maxims. Here, too, the analogy of geology may show that neither formulæ nor maxims are necessary to make a science. Just as a number of skilled geologists, from long experience, agree as to the structure of a complicated piece of country, so will a number of meteorologists agree as to the probable course of any series of cyclones or anti-cyclones.

The limits of a letter do not permit me to show why mean values, or harmonic series can never much advance meteorology as a science, if any better argument were needed than their failure after a trial of forty years.

But I think we are justified in saying that, since the introduction of synoptic methods, meteorology is as much a science as geology; that both are pure observational sciences, and that their methods have much in common, while in some points meteorology is even the further advanced of the two.

21, Chapel Street, S.W. RALPH ABERCROMBY

Atmospheric Currents

I HOPE you will permit me to reply to Mr. Clement Ley's letter on atmospheric currents in NATURE, vol. xv. p. 450.

It is certain that the earth's rotation cannot originate any current, but modifies them when originated.

We are agreed as to the cause of the trade-winds. The controversy is as to the questions, Why the trade-winds do not extend to the poles? What is the cause of the counter-trades or west winds between the trade-wind regions and the poles? and what is the cause of the polar depression of the barometer?

The polar depression of the barometer is due to the centrifugal force of the vortex which is constituted by the counter-trades as they circulate round the pole from east to west. There is a depression at the centre of every vortex, as any one may see in a wash-basin.

The counter-trades are "the reaction of the trade-winds." The laws of motion make it impossible for the winds to have any effect in either accelerating or retarding the earth's rotation—supposing, what we are safe in taking as proved, that they originate exclusively as the effect of solar heat. The effect of the trade-winds alone, blowing from east to west, would be to

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retard the earth's rotation, but this is exactly balanced by the counter-trades of the circumpolar vortexes, blowing from west to east.

I cannot agree with Mr. Clement Ley when he says that it involves a fallacy to explain the mean winds, or great currents, on one principle, and the actual winds, or temporary currents, on another. If the great currents were much feebler in proportion to the temporary currents than they are, the mean prevalence of east winds in the tropics, and of west winds in the higher latitudes, would be discernible only as a residual fact when a number of anemometric observations made at various places were completely discussed. JOSEPH JOHN MURPHY

The Germ Theory

YOUR number for March 22 contains a review of my work on the Germ Theory of Disease, which, in some points, conveys so erroneous an impression of my exact position that I must ask you to allow me space for a few remarks regarding it.

Adverse criticism is what the author of such a book as mine expects, and, to some extent, desires.

A fair representation of his views and arguments, is what every author may insist on as a right.

It is the misleading manner in which my position is stated in your review that has induced me to pen this note.

To one or two of your statements I would refer in illustration of what I complain.

After referring to my expressed belief that contagia are living organised particles—an opinion held by many eminent physicians and men of science—your reviewer says: "If, however, the particles in sheep-pox, small-pox, and vaccine be the infecting matter, they are easily seen by the microscope, and ought, therefore, to be found in the blood, but such is not the case." This, the old and stock argument against the germ theory, is specially dealt with by me on two different occasions—at p. 25, *et seq.*, and at p. 204, *et seq.* If it was worth your reviewer's while to raise this old objection to the germ theory, it was equally worth his while to make some reference to my explanation of the fact on which it rests. This he has not done—a manifest unfairness.

A little further on the review says: "Increased elimination of urea is explained thus: *The increased consumption of liquor sanguinis by the contagium particles leads to increased formation of retrogressive albumen and of urea.* It seems by this that contagium particles have livers and kidneys."

The part which I put in italics is put in your review within inverted commas, conveying thereby the impression that it is a correct quotation from my book. It is far from being so. As given by you, it is a misquoted short passage, separated from its context, and altered to suit the purpose of your review.

The chapter on increased elimination of urea is perfectly clear and intelligible to any ordinary mind, and contains nothing which justifies your reviewer in attributing to me, as he has done, the absurd belief that contagia are possessed of livers and kidneys. Neither is he justified in using the words "eat" and "drink" to express the action of a minute organism on its environment. Such phraseology can serve only to mislead those who are ignorant of the mode in which these organisms grow; and is quite inapplicable to any nutritive process which goes on in such organisms as I have described contagia to be.

Your reviewer quotes my statement that "if we were to bleed, to purge, to give antimony to, or even simply to withhold food and water from all the cases of typhus and enteric fever which occur, there can be no doubt that we should find the mortality from these diseases greatly increased;" and remarks on this, "Dr. Maclagan is right here, for by *simply withholding food and water* there can be no doubt that he would greatly increase the mortality by starving his patients to death." Your reviewer seems to be unaware that I refer in these remarks to a mode of treating fever which at one time did prevail.

To one other point I would refer in illustration of your reviewer's inaccuracy.

He says "the heat of specific fevers is partly ascribed to the propagation of the contagium causing increased consumption of tissue. But increase of living matter causes the disappearance of heat, not its production."

Even according to this, your reviewer's own somewhat awkward statement of the matter, the increased heat is attributed by me to increased consumption of tissue, indirectly brought about by the propagation of the contagium. Nowhere do I say that increase of living matter causes production of heat; and nowhere does your reviewer attribute such a statement to me. Quite the contrary. I distinctly say that the increased production of heat results from increased disintegration of the tissues; and your reviewer distinctly attributes this saying to me. What, then, is the meaning of the latter part of the sentence just quoted? It bears but one interpretation. Your reviewer attaches to the first half of the quotation a meaning the reverse of that which it conveys. While saying that I ascribe the increased heat to increased consumption of tissue, he seems to think that he is saying something quite different, and pens his criticism accordingly. If he thus misunderstands his own statements, I need, perhaps, scarcely be surprised at his sometimes misinterpreting mine. I do object, however, to such misinterpretations and inaccuracies appearing in so influential a journal as NATURE.

Dundee T. MACLAGAN

SEXUALITY IN PLANTS[1]

THE concluding part of the tenth volume of Pringsheim's *Jahrbücher* contains three papers, one of them by Dr. Arnold Dodel, of Zurich, being of the highest importance. This paper occupies the greater part of the present Heft, and is illustrated by eight coloured plates. The title is "*Ulothrix zonata*, its Sexual and Non-Sexual Reproduction, a Contribution to the Knowledge of the Lower Limit of Sexuality in Plants." The anatomy and life-history of the Ulothrix is exhaustively treated, the whole paper being a model of careful and accurate research, as well as a valuable contribution to our knowledge of the lower plants. The paper is divided into sections, of which the following is a short summary. The results given are those obtained during fourteen months' consecutive observation of the plant. The genus Ulothrix has been divided into many species, but Dodel shows that *U. zonata* is so variable in its different stages that most of the so-called species must be reduced to one. The alternation of generations is very remarkable and divisible into four stages. During the progress of the alternation of generation three distinct forms are to be distinguished, two being *filamentous* generations, and the third a zygospore generation. The filamentous generations are invariably produced non-sexually and reproduce themselves repeatedly, forming, in fact, the plant known to systematic botanists as *Ulothrix zonata*. The third generation, the zygospore, was unknown till discovered by Dodel. In the long series of filamentous generations two distinct forms are to be distinguished. The first is produced non-sexually and is the autumn or winter generation. It develops non-sexual macrozoospores and quickly spreads the species in a given locality. The second is a sexual stage developing microzoospores. It arises from the non-sexual macrozoospore, and gives rise to the microzoospores which by conjugation form the third generation, the zygospore or zoozygospore.

The production of the microzoospore-forming generation terminates the series of filamentous generations. This stage is found in spring and summer, and by giving rise to the zygospores which by remaining in a state of rest for some months during the hot dry summer weather, reproduce the plant in the autumn. During the hot weather the filamentous generations more or less completely disappear. The zygospore generation, although a product of gamogenesis, is itself non-sexual.

Ulothrix zonata exhibits polymorphism in a remarkable degree, hence many forms looked upon as distinct species must be suppressed. This opens up a wide question in regard to other algæ, and shows how essential it is to obtain an accurate knowledge of the life-history of all forms.

The cells of Ulothrix give rise to a variable number of zoospores. A mother cell may form 1, 2, 4, 8, 16, or 32 zoospores, there being no obvious distinctions between the sexual and non-sexual zoospores. On the one side is the large macrozoospore with four cilia, and then there is every gradation down to the smallest microzoospore

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[1] Jahrbücher für wissenschaftliche Botanik. Herausgegeben von Dr. N. Pringsheim. Zehnter Band, Viertes Heft. Leipzig: Engelmann, 1876.

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